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THE STORY OF ISOTOPES[1]

By Dr. F. W. ASTON

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THIS chapter in the history of science contains much to interest the philosopher and offers many illustrations of that interplay of theory and experiment by which advance takes place. Theory is the scaffolding of science, and just as in ordinary building operations, though some parts of it may only be used for a short time before removal, others may function for so long a period that they may well be mistaken for the permanent structure itself. The postulate of Dalton (1803) that atoms of the same element are equal in weight is a good example of very permanent scaffolding. For over a hundred years it was practically undisputed and on it was founded the major part of atomic chemistry.

About ten years later Prout made the more speculative suggestion that all atoms were made up of primordial particles which he thought might be atoms of hydrogen. On this view the weights of all atoms must be expressed as whole numbers, and if, as Dalton postulated, the atoms of any particular element were all equal in weight, the atomic weights and combining ratios of all elements must be whole numbers also. Chemists soon found that this was certainly not in agreement with experiment; the more results they obtained the more impossible it was to express the atomic weights of *all* the elements as whole numbers, and of the two theories Prout's was the one to be abandoned. In this decision they were perfectly justified for, as it can not be too often emphasized, it is more important for a scientific theory to be simple than for it to be true. Besides it was of little practical importance to chemists if atoms were not equal in weight so long as in all the ordinary operations of chemistry they behaved as though they were.

[1] Address of the president of the Section of Mathematical and Physical Sciences, British Association for the Advancement of Science, Norwich, September, 1935.

Crookes, however, thought that he had found evidence that they did not so behave, and in his remarkable presidential address to Section B, at Birmingham in 1886, he says: "I conceive, therefore, that when we say the atomic weight of, for instance, calcium is 40, we really express the fact that, while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42, and so on. Later, he developed this idea in connection with his pioneer work on the rare earths. He called the components "meta-elements," but unfortunately for his reputation as a prophet the experimental results on which his idea was founded were later proved to be fallacious, and Dalton's postulate was reinstated as an article of scientific faith more firmly than ever.

Its overthrow, deferred for another twenty years, was one of the many catastrophic results of the tremendous shock due to the discovery of radioactivity. In the rapid development of this, with which the school of Rutherford is so closely associated, the effects of individual atoms, as opposed to those of vast multitudes, were observed for the first time. Chemists could examine elements in the actual process of the making. In 1906 Boltwood observed that his newly discovered element ionium was so similar to thorium that if, by chance, their salts became mixed it was impossible to separate them by any chemical process. Other chemical identities among the products of radioactivity were soon observed and the most painstaking and delicate methods failed to effect or detect the slightest separation.

Discussing these, Soddy, in 1910, boldly stated: "These regularities may prove to be the beginning of some embracing generalization, which will throw light, not only on radioactive processes, but on elements in general and the Periodic Law. . . . Chemical homogeneity is no longer a guarantee that any supposed element is not a mixture of several of different atomic weights, or that any atomic weight is not merely a mean number." The generalization underlying his views was the law connecting radioactivity and chemical change, in the discovery and enunciation of which he played so prominent a part. This law asserts that a radioactive element when it loses an alpha particle goes back two places in the periodic table; when it loses a beta particle it goes forward one place. It follows that by the loss of one alpha particle followed by two beta particles, the atom, though weighing four units less, will have regained its nuclear charge and returned to its original place.

Such changes result in bodies to which Soddy applied the following words: "The same algebraic sum of the positive and negative charges in the nucleus when the arithmetical sum is different gives what I call 'isotopes'[2] or 'isotopic elements' because they occupy the same place in the periodic table. They are chemically identical, and save only as regards the relatively few physical properties which depend upon atomic mass directly, physically identical also." It was fortunately possible to put these revolutionary views to an experimental test in the case of one element—lead, the final inactive product of the thorium and uranium transformations. Uranium of atomic weight 238 loses eight alpha particles to become lead of atomic weight 206, while thorium of mass 232 loses six to become lead of atomic weight 208. Soddy maintained that the lead found in uranium minerals should be lighter, and that in thorium minerals heavier than ordinary lead of atomic weight 207.2.

The complete chemical inseparability of the heavy isotopes formed in radioactive processes passed the most stringent tests and was soon accepted. It was later put to a most ingenious and elegant use by Paneth and Hevesy, who, by adding to an inactive element a small quantity of its radioactive isotope, gave it, so to speak, an indelible label by which its movements and reactions could be followed by the almost infinitely delicate methods of radioactivity. These "radioactive indicators" have been applied to problems of chemistry, otherwise unapproachable, such as the rates of molecular diffusion in the liquid state and the movements of compounds of heavy elements in the sap of living organisms.

The application of the theory of isotopes to elements generally was another matter. The idea that ordinary elements could consist of atoms of different mass received great opposition, for it appeared quite incompatible with such facts as the constancy of chemical atomic weight, the apparently perfect homogeneity of elementary gases and the almost incredible invariability of such accurately measurable constants as the electrical conductivity of mercury independent of its source. This reluctance of orthodox science to accept the theory was, I think, a perfectly natural and healthy reaction. Criticism very seldom destroys enthusiasm and is usually the best stimulant to further research, whereas too immediate a welcome of a new and sensational idea, the outstanding fault of the lay press in dealing with science, may lead to waste of effort. It appears to me a very regrettable thing that, of recent years, it has been repeatedly necessary for experienced research workers to waste their time on the thankless task of disproving the claims of well-

[2] Of recent years the word "isotope" has changed its meaning, and is now used, for lack of another, to designate any atomic species. In the same way the meaning of the word "mass-spectrograph" applied by me to one special type of instrument has now been extended to any form capable of analyzing mass-rays. Such changes, though troublesome, are inevitable for the language of science is a living rather than a dead one.—F. W. A.

meaning victims of self-deception, of whom Blondlot, with his N-rays, is the classical example.

The only satisfactory criterion, a method of comparing the masses of individual atoms, was at the time in process of development. This was Sir J. J. Thomson's "parabola" method of positive ray analysis, and here at first all the results seemed to support Dalton's postulate, indeed the appearance on a sensitive screen of a clear-cut parabolic streak, caused by the impact of the atoms of hydrogen, was the first experimental proof that it was in any sense true of any element, previously it had been purely an article of scientific faith. Hydrogen, carbon, nitrogen and oxygen, present either as atoms or molecules, gave parabolas in the positions expected, and it was only when the rare gas neon was examined that an anomaly was observed. Neon, however pure, always gave two parabolas, a strong one at 20 and a weak one at 22. Referring to the latter in January, 1913, Sir J. J. Thomson said: "The origin of this line presents many points of interest; there are no known gaseous compounds of any of the recognized elements which have this molecular weight. Again, if we accept Mendeléef's Periodic Law, there is no room for a new element with this atomic weight. . . . There is, however, the possibility that we may be interpreting Mendeléef's law too rigidly, and that in the neighborhood of the atomic weight of neon there may be a group of two or more elements with similar properties, just as in another part of the table we have the group iron, nickel and cobalt."

It was my privilege to be associated with him in this work, and as his attenion was fully occupied with the investigation of a parabola of mass 3—now known to be triatomic hydrogen—it fell to my lot to search for a proof that neon was not homogeneous. This I endeavored to do by partial separation of its hypothetical constituents, using as a test its density measured by a quartz micro-balance specially designed for the purpose. The first method, that of fractional distillation from charcoal cooled with liquid air, failed, as we now know was inevitable. The second, diffusion through pipeclay, though extremely tedious, had more success and I was able to announce at the meeting of the association at Birmingham in 1913 that, after thousands of operations, a definite change of density, amounting to about 0.7 per cent., had been achieved. Further data from positive rays were obtained, and, when the war stopped work, there were several lines of reasoning indicating that neon consisted of two bodies of different mass, and that the behavior of these was exactly that predicted by Soddy for isotopes, but none of these was sufficiently strong to carry conviction on so important a conclusion.

During the war Soddy's prediction concerning the atomic weights of leads from uranium and thorium minerals had been triumphantly vindicated by some of his most severe critics, the experts in chemical atomic weights, and when work was started again, although I continued for a time to experiment on separation by diffusion by means of an automatic apparatus, I realized that the most satisfactory proof of the existence of isotopes among the elements in general was only to be obtained by much more accurate analysis of positive rays. This was done by means of a sequence of electric and magnetic fields which gave focussed images of fine collimating slits, thus forming a spectrum dependent upon mass alone. This I called a "mass-spectrograph" (see footnote 2). It had a resolving power of about 1 in 130 and an accuracy of mass measurement of 1 in 1,000. This was ample to prove in 1919 that neon consisted, beyond doubt, of isotopes 20 and 22, and that its atomic weight 20.2 was the result of these being present in the ratio of about 9 to 1. Chlorine was found to contain 35 and 37, and bromine, of atomic weight almost exactly 80, and hence expected to be simple, gave two equally intense lines 79 and 81. Other elements were shown to be much more complex. Krypton, the first of these, had six isotopes, 78, 80, 82, 83, 84, 86; xenon and tin even more. Of the greatest theoretical importance was the fact that the weights of the atoms of all the elements measured, with the exception of hydrogen, were whole numbers to the accuracy of measurement. This "whole number rule" enabled the simple view to be taken that atoms were built of two units, protons and electrons, all the former and about half the latter being bound together to form the nucleus.

Although the interpretation of mass-spectra was often far from simple owing to the difficulty of distinguishing between lines due to compound molecules and those representing true atomic mass-numbers the analysis of the more suitable elements advanced rapidly. Dempster at Chicago discovered the isotopes of magnesium, calcium and zinc by means of an instrument of his own design with semi-circular magnetic focussing. By 1925, when I replaced my first mass-spectrograph, now in the Science Museum, South Kensington, with one of higher resolving power, information on the isotopic constitution of more than half the elements had already been obtained. The new instrument was designed primarily for measuring the minute variations of the masses of atoms from the whole number rule, and had a resolving power ample for the heaviest elements. By its means the search for isotopes has been carried on until a few months ago.

The difficulty of obtaining the necessary rays for analysis varies enormously from element to element. Two main devices are employed: the ordinary gas discharge, which requires the element to be volatile or

form suitable volatile compounds; and the anode ray discharge, in which the halide or other compound of the element is treated as the anode in a discharge at low pressure. The inert gases are particularly suitable to the first method, the alkali metals to the second, other groups of elements being intermediate. Our knowledge of the mechanism of the discharge in both methods is far from complete, so that working with them is still rather an art than a science. The element of luck has played an important part in cases where the properties of the materials are unfamiliar and unfavorable to the conditions of the discharge.

The analysis of the recently discovered element rhenium offers a good example. The only available volatile compound was the heptoxide, a sample of which has been kindly provided by the discoverer Noddack. The vapor of this crystalline solid was first admitted to the discharge bulb, but without success. The solid was then introduced into the bulb itself, and, although its vaporization was so copious that a visible layer was formed on the walls, still no lines were obtained. At this stage the element was abandoned as quite hopeless and preparations were made to go on to another. Purely by chance, this happened to be gold, which it was intended to attack by means of its slightly volatile chloride. This compound gives off chlorine gas when heated, and, as previously it had been noticed that the presence of a halogen gas often stimulated the appearance of lines otherwise faint, it was considered just worth while to make one trial with it *before* the rhenium oxide deposit had been cleaned off the walls. This was successful beyond all hopes. No lines of gold were found, but the rhenium doublet appeared in great strength, giving convincing evidence that it consisted of two isotopes, 185 and 187.

The technique of anode rays is, if anything, even more capricious but, when successful, yields spectra almost free from the lines of compounds and is for this reason particularly suitable for the identification of new isotopes. This method has been recently applied to the large group of the rare earth elements yielding some thirty new isotopes.

From the point of view of the identification of the more abundant isotopes our knowledge is nearly complete. A year ago only four elements, palladium, iridium, platinum and gold, remained without mass-spectrograph data. Dempster has since developed an entirely new method of obtaining suitable rays by using a very intense spark discharge, and I have just heard from him that he has already identified five isotopes of platinum and one of gold. It seems very probable that the last two elements will have yielded before this address is delivered.

In all some 253 stable isotopes are known of which seven were discovered by observations on optical spectra, and have since been confirmed by the mass-spectrograph. This large assembly shows many empirical laws, of which perhaps the more remarkable is that no odd-numbered element, with the possible extremely rare exception of the isotope of hydrogen of mass 3, has more than two isotopes. Even elements are not so limited. The most complex element so far observed is tin, with eleven isotopes ranging in mass number from 112 to 124. One of the most astonishing results is that, for practically every natural number up to 210, a stable elementary atom is known, many are filled twice over and a few three times with "isobares," that is, atoms of the same weight but different chemical properties. Schemes of tabulation of all the known species have led to the prediction of isotopes and to theories of nuclear structure to account for their occurrence.

Study of the relative abundance of isotopes in the mixture we still call, for convenience, an element is of interest from two entirely different points of view. In the first place, since it appears to be perfectly invariable in nature, not only in terrestrial but also in meteoric matter, there was a slight hope that a systematic measurement of abundance ratios might disclose some simple relations bearing on the great problem of how the nuclei of atoms were evolved. The relative abundance of isotopes can be estimated by several methods, but that of the most general application is the photometry of mass-spectra. A technique of this was worked out in 1929, and a number of elements examined, but the ratios, obtained in numbers large enough for statistical treatment, showed no groupings other than would have been expected from pure chance. These measurements have a second important practical value. If we know the masses of the isotopes of an element and their relative abundance it is easy to calculate their mean weight. This, with proper corrections, can be used to check the chemical atomic weight. During the past six years nearly every atomic weight has been determined by this purely physical method, which has the great advantage of being, in general, independent of purity and requiring an almost infinitesimal quantity of material.

Instead of the original view that the nuclei of atoms consisted of protons and electrons, it is now considered more likely that they are built of protons and neutrons. In either case the binding forces holding the particles together must represent loss of energy, that is, loss of mass. Hence it is that the atom of hydrogen has abnormally high mass and that the accurate determinations of divergences from the whole number rule are of such profound theoretical importance. As I have stated, my second mass-spectrograph was designed for

this and found capable of an accuracy, in favorable cases, of 1 in 10,000. The atom of oxygen 16 was chosen as standard and the percentage divergences expressed in parts per 10,000, called "packing fractions," were determined for a large number of elements. These, when plotted against mass number, were found to lie roughly on a hyperbolic curve. This drops rapidly from hydrogen, passes through a minimum of about −10 in the region of iron and nickel, and then rises gradually, crossing the zero line in the region of mercury. Our knowledge in this field has been notably increased by the brilliant work of Bainbridge, who set up at Swarthmore a powerful mass-spectrograph of an original design which made use of a velocity selector and semicircular focussing. With this instrument he discovered new isotopes of tellurium, rectified results on zinc and germanium, and has made many of the most accurate comparisons of mass so far known.

Fortunately for these comparisons, and particularly so for the extension of an accurate scale of mass to the heavy elements, particles occur in the discharge which carry more than one positive charge. A particle with two charges will give a line corresponding to half its mass, one with three charges will have an apparent mass of one third, and so on. These lines are called lines of the second, third and higher orders. The complex element mercury seems specially provided by nature to help in the work. Not only do its nine isotopes provide a most valuable scale of abundance but it usually occurs in the discharge, to which its presence is advantageous from the point of view of smooth running, and it is unique in its property of forming multiply charged ions. Mercury lines up to the fifth and sixth orders can be detected so that it provides a perfect natural scale, a link between light and heavy atoms absolutely necessary to extend accurate measurements to the latter. The packing fraction of mercury, which is practically zero, was determined by means of its third order line $^{198}Hg^{+++}$.

Of the recent episodes in the story I relate certainly the most sensational is the discovery of deuterium, the heavy isotope of hydrogen. The events leading up to this and following it form a most remarkable sequence. In them the elements of nature seem to have joined in an impish, but fortunately benign, conspiracy to delude the observer and to turn his most sober researches into a sort of blind man's buff.

The first comparison of the masses, now termed "isotopic weights," of the atoms ^{1}H, ^{12}C, ^{14}N and ^{16}O to a high degree of accuracy were made with my second mass-spectrograph and published in 1927. Various methods were used which can not be given in detail here, but since the comparison of H with O could only be done through the intermediate ^{4}He, and even then the ratios measured were very large, little reliance could have been placed on the figure for H unless it could be checked in some quite independent manner. It was possible to do this by means of the close doublet $O—CH_4$, and, when measurements of this appeared to support my values for C and H, I had no reason to doubt their substantial accuracy. In this I have been justified to some extent, for the figures have stood for seven years, and a direct determination of the He,H ratio, made later by Bainbridge, agreed exactly with mine. A further support was afforded by the fact that the figures for the four elements, all then supposed to be simple, agreed within 1 or 2 parts in 10,000 with the accepted chemical atomic weights.

This satisfactory agreement was completely upset in 1929 by the startling discovery of the heavy isotopes of oxygen 17 and 18 which, present in small quantity, had naturally been overlooked on mass-spectra of that element, owing to the technical difficulty of ensuring the absence of the isobaric compound lines OH and OH_2. The discovery was made by Giauque and Johnson by observations on band spectra, which are free from this confusing disability, and the careful quantitative work of Mecke, made later, showed that, owing to the presence of these isotopes, the chemical standard of atomic weight O = 16 was about 2 parts in 10,000 heavier than the physical one $^{16}O = 16$. Examination of compounds of carbon and of nitrogen by the same method showed not only that these elements also contained heavy isotopes ^{13}C and ^{15}N but that their apparent abundance, by a most incredible coincidence, was just about enough to bring their mean weights into line with that of oxygen.

Birge pointed out that to satisfy my low estimate of ^{1}H hydrogen must also contain at least one heavy isotope. Urey took up the problem and, happily unaware of the real uncertainty in the figures concerned, with the collaboration of Brickwedde and Murphy fractionated liquid hydrogen and proved by examination of the Balmer lines that ^{2}H was present. Washburn showed that its heavier atoms could be concentrated by the electrolysis of water. This method was developed so rapidly and brilliantly by Lewis that, soon after its discovery, pure heavy water had been obtained in appreciable quantity. The isotope of hydrogen of mass 2 can not be treated as a normal isotope. Its exceptional difference in mass enables it to be separated with comparative ease in a pure state. It has been given the name deuterium, symbol D, and heavy water D_2O is now obtainable in quantity at reasonable prices, one of the most surprising reagents in the history of science and certainly one which would have dismayed the founders of the C.G.S. system of units.

Now comes the interesting sequel. Deuterium and its triatomic molecule supply two links, missing before, of the three forming a closed chain of masses by which H can be directly connected with O, given a mass-spectrograph of sufficient resolving power. These links are the doublets D—H_2, at mass 2, C^{++}—D, at mass 6, and O—CH_4 at mass 16. By means of an improved collimator I have recently increased the resolving power of my mass-spectrograph to that necessary to achieve at least a partial separation of the extremely close doublet D—H_2, and to make a much more accurate estimate of the doublet O—CH_4. The latter has disclosed the disturbing fact that this is really wider than I had taken it to be and so no longer confirms the early value of C and H. Provisional work on the wide doublet C^{++}—D_3 makes it reasonably certain that my original value for H is 2 or 3 parts in 10,000 too low, as is also suggested by nuclear transformation experiments. Here we have the pretty paradox of the element discovered providing the means to remove that very discrepancy which seemed to point the way so clearly to its discovery. In view of its valuable results I am not likely to regret my mistake, however serious it turns out to be. The only moral to be drawn from this seems to be that you should make more, more and yet more measurements. Even a bad one *may* be of service, but, fortunately, it will be essential for you to make a considerable number of good ones first, or no notice will be taken of it.

In the field of isotopes, as in so many fields of physical and chemical research to-day, the objective we now aim at is the next decimal place, an elusive object which always appears to be running away from the observer, like a distant spiral nebula. The need for isotopic weights of the highest accuracy is urgent. In artificial radioactivity and transmutation we see the real beginnings of a great new subject, the nuclear chemistry of the future. Its equations can only be founded securely upon direct determinations of masses by the mass-spectrograph, and the nuclear chemist already demands these to an accuracy of 1 in 100,000. I have little doubt we shall be able to provide him with these in the course of a year or two. Armed with reliable equations, and thereby with more and more definite knowledge of nuclear construction, he will transmute and synthesize atoms as his elder brother has done molecules, with results to be wondered at and possibly even misused by his fellow creatures. I foresee a time, not immeasurably far distant, when it will be possible for us to synthesize any element whatever, wherever and whenever we please; alchemy indeed in the service of man.

THE FIFTEENTH INTERNATIONAL CONGRESS OF PHYSIOLOGY

THE CONGRESS AND RUSSIAN PHYSIOLOGY

NEARLY one thousand persons (not all physiologists!) have just returned from a visit to the Soviet Union after attending the fifteenth International Physiological Congress. Every conceivable kindness was shown and honor done to us, and we had the best possible opportunity (in the limited time available) of making ourselves acquainted with Russian physiologists and physiology. We came away filled with affection and regard for our Russian colleagues and deeply touched by their welcome; glad indeed to witness the respect with which they, as scientists, are treated in their country and to appreciate the important position which science, pure as well as applied, and particularly physiology, occupies in the economy of the Soviet Union; moved by the ardor and enthusiasm of the army of young scientific workers; looking forward with hope, not perhaps unmixed with trepidation, to the contributions which these will make to our subject in the next twenty years.

Those twenty years will be critical, and their result will depend very largely upon a factor of which, as yet, the youthful ardor of Russian science takes little obvious account. The devotion of the state to science is proclaimed, and huge resources are lavished upon scientific work. Great new laboratories and scientific establishments are being built and equipped. In a few years there will be literally thousands of enthusiastic workers in physiology and its allied sciences—there were said to be five hundred at the congress. With all this devotion, with all these resources, with all this faith in science—not only for its practical uses, but as an instrument of human culture—is it possible that anything can be missing? One wonders.

In Great Britain and America we are not unacquainted with this problem. The state, as state, may not devote so large a fraction of its resources to the encouragement of scientific work, but our traditions and our particular forms of government—imperfect as in many respects they may be—do in fact allow other and not inconsiderable resources to be so used. The Rockefeller Foundation, the Commonwealth Fund, the buildings and endowments of innumerable universities and institutions of teaching and research throughout America; the ancient and other foundations of the schools and colleges and universities in Great Britain, the endowments of the Royal Society and of various

other bodies, the vast resources of many private agencies available throughout the British Empire for research; these, and many other such, certainly no less in total amount than those devoted to science in the Soviet Union, have given us, over a long period of years, a fair amount of experience in the administration of funds for scientific research and development. We know well that laboratories, equipment, thousands of enthusiastic and devoted workers, even public honor paid to scientists (not quite negligible, even in capitalist England!) are not enough. With all these in full measure, there may still be something missing. We know it only too well.

A proper degree of scientific skepticism, a stern capacity for criticizing the work of oneself and others, a habit of rigorous "control" of experimental results, a severe standard in scientific accuracy of experiment and thought, these are fundamental conditions of high scientific achievement. Without them vast resources may be wasted, great and idealistic plans may be brought into contempt, huge unstable edifices may be erected. Are these critical factors, by some political magic, inevitably present in the science of the Soviet Union? Some of us wonder. If not, can we look to the future of Russian science with quite unmitigated hope?

These doubts are expressed in all friendliness, in the desire to help. One would hate to see "a good ship spoilt for a ha'p'orth of paint." But science is like a chain, built up one link on to another; if one is bad the chain may be worse than useless. Each link separately must be examined and tested; it must be mistrusted until it is proved good. Can we be sure that the present environment of Soviet science is one which breeds this necessary skepticism? If not, may the following suggestions, of which the third is far the most important, be of service?

I. That publication, at least of a significant fraction of the more important papers, should be made in some language which the rest of the world can read, preferably in English, which is more widely read than any other language. The Japanese publish journals in English and German, the Italians in French, the Scandinavians in English and German, the Dutch in English and French: there are many such examples. At present the physiological world does not know what is being done in Russia. We can neither appreciate nor criticize, yet both appreciation and criticism are needed in a rapidly developing science. Why not publish in English, or if necessary in French or German, a Russian physiological journal, to be circulated outside the Soviet Union as well as inside, not for purposes of propaganda, but as an ordinary scientific publication, to allow our colleagues to bring their ideas and work readily to the notice of the world, to join as partners in criticism and discussion with the rest of us?

II. That, conversely, our Russian colleagues should be given the financial means of purchasing abroad the journals, at least the most important journals, of the rest of the world, together with sufficient text-books and monographs on special subjects. At present, in many places at least, they are being intellectually starved for lack of foreign publications.

III. That the ablest of the younger scientists should be sent, as a matter of course, outside the Soviet Union to study in other countries, not merely for a month or two but for a sufficient period to enable them to make proper scientific contacts, to learn what is being done and to pick up some of the scientific skepticism which is traditional in the older scientific schools. They long for the opportunity to work abroad. They are pathetically eager for the wider contacts. They told one that it seemed like a dream to see and talk with Barcroft, Cannon, Gasser, Adrian. Their eagerness and their imaginative qualities would make them welcome in any foreign laboratory, and they would return all the better equipped to build up Russian science on sound and critical lines.

The government of the Soviet Union believes in international cooperation. Will they not make these fundamental contributions to Russian and so to international science? The reason for not sending their young scientists to study abroad has been alleged to be the difficulty of finding the necessary *valuta,* but the trade balance is not really so bad as that now, and when fellowships were available, through one of the great international foundations, a few years ago, they were not used. No country can be, or can dare to try to be, self-sufficient in science, particularly when developments are taking place so fast. The fear, which perhaps underlies the inhibition, that the pure faith of the young scientists from the USSR might be corrupted in other lands, is an unnecessary one: we do not subject our guests, at least in England and America, to the dismal drone of political propaganda. One can not doubt the genuine and earnest wish of the government of the Soviet Union for closer and friendlier relations with other countries, or their sincere appreciation of science, or their desire to forward science, pure and applied, in their own country and so in the rest of the world. By their extraordinary courtesy to their thousand guests of the Physiological Congress they made all this abundantly clear. Why, therefore, should they not take these three simple practical steps to fulfill their admirable purpose?

A. V. HILL

LONDON, AUGUST 26, 1935

PROCEEDINGS OF THE CONGRESS

THE fifteenth International Physiological Congress, which took place in Leningrad and Moscow from August 8 to 18 under the presidency of Professor I. P. Pavlov, was in many respects a unique occasion. It marked the first time that this or any other large in-

ternational scientific congress has been held in the USSR, and for the first time physiologists from all parts of the world obtained first-hand contact with the work of their Russian colleagues. Further, the excellence of the entertainments and general arrangements, as well as the great interest of the new social developments of Soviet Russia engrafted upon the old culture, made the event a memorable one for all privileged to attend.

The opening events took place in the large hall of the Uritzky Palace, formerly a royal residence and later the meeting place of the State Duma, but now part of the university system of Leningrad. The great hall, which had been newly renovated and was decorated with flowers and large palms, was a brilliant scene as the membership convened. Head phones were provided at every seat through which one could listen to the speakers directly, or plug in for a translation in Russian, German, French or English. General comment was made on the satisfactory operation of this system. Professor Pavlov declared the congress open and welcomed the visiting members to the Soviet Union in the address which follows:

I declare the session of the fifteenth International Congress of Physiologists open. I am greeting here in the name of all the physiologists of my country our dear comrades who have come here from every part of the world, and I heartily wish them to spend the time here usefully and pleasantly.

This congress of physiologists, which is the fifteenth in number, is the first to meet in this country. This is as it should be. Ours is a young physiology. We are only the second generation of Russian physiologists, although this generation is nearing the end of its days. We must regard as the father of our physiology Sechenov, who was the first Russian to deliver his lectures, not from somebody else's book but as a specialist with his own demonstrations, and who established the first physiological school in this country. All this was due of course to his outstanding abilities. That is why we deemed it appropriate to present to the members of the congress his best works and a medal with his likeness. Sechenov was the promoter of physiological works on a large segment of the globe.

The manifold benefits of international congresses are so obvious and have been so much talked of that I shall emphasize only a few points which are of special significance in the present case.

It is time for us, physiologists, as has already been stated among us on numerous occasions and as is being practiced at other congresses, finally to adopt a decision on what is known as questions of program, that is, questions which provoke a particularly lively interest at the moment, perhaps coupled with a limited number of communications on special subjects. General meetings ought to be held, to which should be invited both persons investigating the question selected and disputants. In such instances of deliberate and stimulating discussions, special remarks from colleagues not directly connected with the question may prove to be of considerable value.

The second point which I wish to raise as being of particular importance to us at the present time is the question of the special influence which such meetings of scientists exercise over the young generation, over the young scientists. I know the power of this influence from my own experience in my young days, from the old congresses of our naturalists and physicians. Our government is now spending very large sums for scientific research and is drawing masses of young people into science, and the spectacle of world scientific achievements embodied in living persons must have an enormous stimulating effect on these young people.

Finally, point 3. We are all different people, yet at the moment we are united and stimulated by a keen interest in the common task of our lives. We are all of us good comrades, with clear bonds of friendship uniting many of us. We are apparently working for a rational and final unification of mankind. But should a war break out many of us will enter into hostile relations with each other, and precisely in our scientific sphere, as has happened more than once. We shall not want to meet together as we are doing now, even our mutual scientific appraisal of one another will become different. I can well understand the greatness of a war for liberty. At the same time, it can not be denied that war is essentially a beastly method of solving life's difficulties [applause], a method unworthy of the human mind with its immeasurable resources. At the present time there is to be observed an almost world-wide desire and intent to avoid wars, perhaps by more certain means than in the past and I am happy that the government of my mighty fatherland, in its fight for peace, was the first to declare for the first time in history: "Not one inch of foreign soil" [stormy applause]. We must particularly sympathize with and promote this, of course. And as seekers of truth we must add that it is necessary strictly to observe justice in international relations. This is the chief real difficulty.

This year our active world society has lost two true members. There has passed away Professor Schaeffer, formerly of the University of Edinburgh, who had devoted all his long life very successfully to our science. There has also passed away at the height of his working powers Professor Macleod, of Aberdeen University, who had been crowned with a Nobel prize. Let us honor, by rising, the memory of the comrades who have passed away from us. [All rise and the Chopin funeral march is played.]

In conclusion, we, native physiologists, must tender thanks to our government which has given us the opportunity to accord a worthy reception to our dear guests. I am giving the floor to the chairman of the government commission specially appointed for assisting the organization of the congress.

Following the address of the president, I. A. Akulov, secretary of the Central Executive Committee of the USSR, delivered a speech on behalf of the Soviet

Government. I. S. Kadatski, chairman of the Leningrad Soviet, welcomed the members on behalf of the city Soviet and the workers of Leningrad. Greetings were also delivered by Professor Karpinsky for the Academy of Sciences. The principal address was by Dr. W. B. Cannon, of Harvard University, who spoke on "Some Implications of the Evidence for Chemical Transmission of Nerve Impulses." A second plenary session was held in the same room a few days later, at which Professor L. A. Orbeli, of Leningrad, spoke on "Pain and Its Physiological Effects" and Sir Joseph Barcroft, of the University of Cambridge, on "The Velocities of Some Physiological Processes."

All the scientific sessions were held in the Wiborgsky Dom Cultury, where the various sections met in convenient relation with each other. A total of forty morning or afternoon sessions were held, including some which consisted of demonstrations and moving pictures. Five sections met simultaneously, each including a group of papers having a more or less common interest. These programs suffered from the usual deficiencies incident to such occasions, where many languages are spoken and where a large number of brief communications on varied topics are read. However, the sessions in general were well attended and the standard was not below that set by recent meetings of the congress.

Outside the lecture halls in a large number of rooms of the Wiborgsky Dom Cultury there was arranged an elaborate series of exhibits depicting the development of the science of physiology in the USSR, and graphically showing, in an excellent series of charts, representative results of recent investigations by Russian workers. This demonstration portrayed in a vivid manner the enormous expansion in the facilities for physiological research and the increase in the number of investigators which have taken place within a very few years. For example, it is stated that in tsarist Russia there were only 24 institutions for physiological research, whereas the Soviet Union already has 380 institutions working in the field of physiology. Similarly, Russian workers in physiology have greatly increased, and they are said now to be numbered in thousands, of whom over 500 were present at the congress. The rate of expansion has been so great that many of us have felt that there is danger of a loss in critical judgment and a lowering in the standards of scientific work—a feeling which is voiced by Professor Hill in this number of SCIENCE.

Most of us have heard of the alleged conflict between science and religion, but to the visiting members of the congress a more serious conflict was that between science and the many forms of entertainment which Russian hospitality provided. It is the latter phase of the congress which has left the most enduring impressions on the visiting members. Private cars were provided between the hotels and meeting places as well as for daily excursions. Passes were issued giving members free use of street cars and busses. In fact, the membership badge or the word "congress" was effectively the key to the cities of Leningrad and Moscow, giving free access to the picture galleries, museums, parks, palaces and other public buildings. The official entertainments, of which there were a large number, were on a scale and of a magnificence which but few of us had previously experienced. Among these may be mentioned the opening reception in the Ethnographical Museum given by the Physiological Society of the USSR, which was attended by more than 1,200 members and Soviet officials. The display of food and refreshments in the beautifully decorated marble hall will long be remembered. The official banquet, which was given at Detskoye Selo, 15 miles from Leningrad, in the throne room of the Catherine Palace, was a scene of great splendor, said to be strongly reminiscent of the elaborate feasts of the tsarist régime. The banquet, lasting many hours, was followed by an impressive display of fireworks in the garden, after which there were further refreshments and dancing in the Cameron Gallery. Another event of great interest, which may be mentioned now, was the reception given later in honor of the delegates of the congress by V. M. Molotov, chairman of the council of the peoples' Commissars of the USSR, in the Grand Kremlin Palace in Moscow, at which over 1,500 people were present. On this occasion Mr. Molotov made a speech describing the position and importance of physiology in the Soviet Union. Replies were made by the president, Professor I. P. Pavlov, and various other members of the congress, including Andres Mayer (France), Otto Loewi (Austria), A. V. Hill (England), U. Ebbecke (Germany), H. J. Jordan (Holland) and I. P. Rasenkov, chairman of the All-Union Physiological Society. The reception ended with a concert, in which the actors from Moscow theaters and the amateur art group from the Central House of the Red Army participated. In Leningrad also a special concert was given for the membership of the congress in the state theater of drama, in which many of the outstanding artists of Russia took part.

Facilities were provided for visiting the numerous laboratories and medical institutions in Leningrad and Moscow, as well as the various government enterprises and social institutions developed for the furtherance of educational and recreational opportunities for the workers. These various "extracurricular" activities gave abundant opportunity for getting in touch with the scientific work going on in Soviet Russia and the opportunity to meet investigators in their own laboratories and of talking with them during excursions

and other entertainments, and it is perhaps from this source that the most important fruits of the congress are to be expected. In the past Russian physiology has developed with very little interchange with other countries, and if the contacts made during the congress are further developed, as they almost certainly will be, it may well mark an important milestone in the advance of physiology.

The scientific sessions in Leningrad ended on August 16, and during the evening the entire membership of the congress entrained for Moscow, where the final plenary session was held on the following day. At this meeting, held in the great hall of the Conservatory of Music and presided over by Professor Pavlov, Professor Lapicque delivered an address on "Recent Progress in the Study of Nerve Function" and Professor Ukhtomski on "Physiological Lability and the Act of Inhibition." Professor Hill then presented the report of the International Committee, the substance of which follows:

(1) The retirement of W. H. Howell, Baltimore, and I. E. Johanson, Stockholm, was announced with regret. These vacancies were filled by the election of W. B. Cannon, Boston, and G. Liljestrand, Stockholm. The other members of the committee are: F. Bottazzi, Naples; O. Frank, Munich; A. V. Hill, London; L. Lapicque, Paris, and I. P. Pavlov, Leningrad.

(2) It was decided to accept the invitation of Professor Hess, of Zurich, and the Society of Swiss Physiologists to hold the 1938 congress in Switzerland, a decision which was unanimously ratified by the general session. A reminder has been received from the Hungarians to the effect that an invitation to Hungary still exists and will be renewed in 1938. Reference was made to the fact that the first International Physiological Congress was held in Switzerland in 1889, so that the next meeting will effectively be the jubilee of the first congress.

(3) The International Committee recommended to the Swiss National Committee that applications for membership should not be accepted directly from individuals, but only through committees of national physiological organizations. This recommendation was made for the purpose of limiting membership in general to investigators in laboratories of physiology, pharmacology, biochemistry and experimental zoology.

(4) The International Committee decided that the next national organization should be advised of the strong opinion of the committee that the arrangements of future congresses, particularly in regard to entertainment, should be simplified so that not too great a financial burden should have to be faced by the national committee. The membership fee should as far as possible cover the cost.

(5) It was decided to recommend to the next national committee that experiments be made on the matter of improving the programs. The method proposed is to arrange that printed communications with suitable references supplied by registered members should be circulated some months before the meeting. Any member might signify his wish to take part in a discussion, which would be arranged in subject groups having a common interest. Discussion only and not presentation would be allowed.

(6) The function of the International Committee had been in practice advisory, but if, for any reason, any national committee should resign, temporary executive functions would be taken on, pending the formation of another national committee. To secure permanency a permanent officer is required and the committee decided to appoint a permanent secretary to act as their center between meetings and to conduct correspondence with the national committee. Professor A. V. Hill, of University College, London, was asked to act in this capacity. Any correspondence to the international committee can be sent to the secretary, but business of the next congress will be in the hands of the Swiss committee.

Professor Hill expressed the deep gratitude of the international committee to their Russian colleagues for the warmth and wealth of their welcome and then called upon Professor Barger, of Edinburgh, to voice the opinion of the congress membership. This Professor Barger did most acceptably and to the admiration of all present in eight languages—Russian, English, German, French, Spanish, Italian, Dutch and Swedish. After the business session was concluded, Professor Pavlov closed the congress with a few words, thanking its members for their cooperation in the work of the sessions and expressing his gratitude that the congress was held in the Soviet Union.

McKEEN CATTELL

LONDON, AUGUST 28, 1935

SCIENTIFIC EVENTS

FELLOWS OF THE BEIT MEMORIAL FELLOWSHIP FOR MEDICAL RESEARCH

AT a meeting of the business department of the Beit Memorial Fellowships in London on July 12 it was reported that fellowships during the year 1934–1935 were held by 26 workers. Among the appointments gained during the year by past fellows may be noted that of J. R. Marrack (1914) to the professorship of chemical pathology, University of London, at the London Hospital; of A. St. G. Huggett (1922–1925) to the professorship of physiology, University of London, at St. Mary's Hospital; of W. P. Kennedy (1929–1932) to the professorship of biology, Royal Medical College, Baghdad, and of F. H. Smirk (1930–1934) to the professorship of pharmacology in the

Egyptian University, Cairo. W. R. Aykroyd (1928–1931) has been appointed director of Nutritional Research under the Indian Research Fund Association. S. Zuckerman (1934–) was awarded the W. J. Mickle prize fellowship for 1935 of the University of London.

The following elections were made:

Senior Fellowship (value £700 per annum).

Robert Hill, M.A. (Cambridge).—To continue his studies on the respiratory function of hemoglobin. At the Physiological Laboratory and the Molteno Institute, University of Cambridge.

Fourth Year Fellowships (value £500 per annum).

Robert Gaddie, B.Sc., Ph.D. (Edinburgh).—To continue his research on the metabolism of muscle. At the Departments of Medical Chemistry and Materia Medica, University of Edinburgh.

John Michael Robson, D.Sc., M.D. (Leeds).—To continue his work on the hormonic factors concerned in the maintenance of pregnancy and initiation of parturition. At the Institute of Animal Genetics, University of Edinburgh, or the School of Agriculture, University of Cambridge.

Frank George Young, B.Sc., Ph.D. (London).—To study the diabetogenic factors of the anterior pituitary gland. At the Department of Physiology and Biochemistry, University College, London.

Junior Fellowships (normal value £400 per annum).

Alexander Robertus Todd, B.Sc. (Glasgow), Ph.D. (Frankfurt), Ph.D. (Oxford).—First Class honors B.Sc. Chemistry; Carnegie Research Scholar, 1928; Senior Studentship, 1851 Exhibition. Published work—chemical papers on the structure of bile acids, the pigments of plants and the biochemistry of micro-organisms. Proposed research—the molecular structure of vitamin B_1.—At the Department of Medical Chemistry, University of Edinburgh.

Robert John Kellar, M.B., Ch.B. (Edinburgh), M.C.O.G.—Annandale Gold Medal in Surgery; Murchison Scholarship in Medicine; Freeland Barbour Research Fellowship, 1933–34; Leckie-Mactier Fellow in Medical Research, 1934–35. Published work—on cortical necrosis of the kidneys during pregnancy, and on ovarian pregnancy. Proposed research—the problem of nephritis and high blood pressure associated with pregnancy. At the London laboratories of the Royal College of Surgeons and the Obstetric Unit, University College Hospital, W.C.1.

Norman Lowther Edson, M.B., B.S. (New Zealand).—New Zealand Graduate Medal in Senior Clinical Medicine; Medical Traveling Scholarship, University of New Zealand, 1930. Published work—biochemical papers on catalytic hydrogenation, etc. Proposed research—fat metabolism and ketogenesis. At the Institute of Biochemistry, University of Cambridge.

Myer Head Salaman, M.A., M.D. (Cambridge).—Anderson Prize for Clinical Medicine, London Hospital; House Physician, 1931; research on virus diseases under Professor S. P. Bedson, F.R.S., 1932–34. Published work—experiments in peripheral vision; the flocculable substance of vaccinia. Proposed research—investigation of the antigenic structure of the vaccinia virus. At the Lister Institute of Preventive Medicine, London, S.W.1.

James Davidson Fulton, B.Sc. (Distinction in Chemistry), Ph.D., M.B., Ch.B. (Glasgow).—Carnegie Scholar, 1924; Ramsay Fellow, 1926–28; Chemist to the Manchester Committee on Cancer, 1928–30. Published work—biochemical papers, especially on the nature of carcinogenic agents in mineral oils. Proposed research—the experimental chemotherapy of malaria. At the London School of Hygiene and Tropical Medicine, W.C.

Adele Helen Rosenheim, M.A. (Cambridge), Ph.D. (London).—Mary Sparke Scholarship, Newnham College, Cambridge, 1929; Grocers' Company Research Scholarship, 1931–33. Published work—biochemical papers on the calcification of cartilage and bone. Proposed research—the chemical nature of antibodies, especially those in antityphoid sera. At the Lister Institute of Preventive Medicine, London.

All correspondence of fellows and candidates should be addressed to Professor T. R. Elliott, M.D., F.R.S., Honorary Secretary, Beit Memorial Fellowships, University College Hospital Medical School, University Street, W.C.1.

THE SEVENTY-FIFTH ANNIVERSARY OF THE COLUMBIA SCHOOL OF MINES

A COMMITTEE has been appointed to make plans for the celebration in 1939 of the seventy-fifth anniversary of the Columbia School of Mines, the first institution of its kind to be established in this country.

The celebration will direct attention to New York City as the greatest center of mining corporations and mining management in the Western hemisphere and to Columbia University as the pioneer in providing organized education for leadership in the mining industry.

The founder of the Columbia School of Mines was Thomas Egleston, Jr., a portrait of whom, by Michael de Santis, was recently hung in the Egleston Engineering Library of the school. Egleston was born in New York City on December 9, 1832, of an old Massachusetts family, to which belonged Generals Egleston and Patterson, distinguished in the Revolutionary War. He died on January 15, 1900.

In March, 1863, Professor Egleston published a "Proposed Plan for a School of Mines and Metallurgy in New York City." It was adopted by the Columbia Trustees and on November 15, 1864, the school was opened in the basement of the old college building at 49th Street. Mr. Egleston, a graduate of Yale University, who had previously been in charge of specimens at the Princeton Institution, was made professor of mineralogy and metallurgy without salary. Francis L. Vinton became the first professor of mining engi-

neering, and Charles F. Chandler, pioneer in industrial chemistry, was appointed dean of the faculty and professor of analytical chemistry. Various officers of the college volunteered their services. Torrey lectured on botany, Joy on geology, Peck on mechanics and mine surveying, Reed on physics, Van Amringe on mathematics.

Though founded as a School of Mines, provision was early made for courses leading to the Ph.B. and the C.E. degrees; later architecture, mechanical engineering and electrical engineering courses were added, and it became eventually the School of Engineering of Columbia University.

Though academically a part of the School of Engineering, the title School of Mines is still borne by the mining and metallurgical departments. Pioneer work is still being done. A separate course in mineral dressing, with provision for advanced instruction was recently established, and the first laboratory equipped to make tests upon underground excavations, considered as structures, has also recently been established there.

The School of Mines has a long list of distinguished graduates. Among those prominent in fields other than mining and metallurgy are Irving Langmuir, '03, director of research of the General Electric Company and Nobel prize laureate in 1932; H. Hobart Porter, '86, president of the American Waterworks and Electric Company. The late Nathaniel Lord Britton, '79, for many years director-in-chief of the New York Botanical Garden, and Carl Barus, '77, professor emeritus of physics at Brown University and formerly dean of the Brown Graduate Department.

THE SILLIMAN LECTURES AT YALE UNIVERSITY

THE twenty-sixth series of lectures on the Mrs. Hepsa Ely Silliman Foundation will be given at Yale University during October by Dr. Edwin Powell Hubble, astronomer of the Mount Wilson Observatory, Pasadena, California. The series, entitled "The Realm of the Nebulae," will consist of eight lectures given on Mondays, Wednesdays and Fridays, at 4:15 P. M., in Strathcona Hall, beginning on October 14.

The lectures will present the available information derived from direct observations in the observable region of space considered as a sample of the universe. They will sketch the formulation of the problems and indicate the nature of the material, the methods which have penetrated the field and the results to which they lead. Finally, the results will be compared with those to be expected in current theories of cosmology.

Following are the subjects of the individual lectures:

1. The Exploration of Space
2. Classification of Nebulae
3. Distribution of Nebulae
4. Distances of Nebulae
5. The Velocity-Distance Relation
6. The Local Group
7. The General Field
8. The Realm of the Nebulae

The subject is one of compelling scientific interest and will be handled by one who is a recognized leader in this field. Dr. Hubble is not only a master of the current theories of cosmology, but has with his associates at Mt. Wilson made the observations upon which these theories, particularly those of Einstein, deSitter and others, have been based.

SCIENTIFIC NOTES AND NEWS

DR. HOWARD MCCLENAHAN, secretary of the Franklin Institute and director of the museum and of the Benjamin Franklin Memorial, has resigned as director but will continue as secretary of the institute, a position that he has held since 1925. Henry Butler Allen, chief metallurgist of the Henry Disston Company, has been elected director and will take up the work on October 1. Dr. McClenahan has been granted a year's leave of absence, which he plans to spend in travel abroad.

AT a meeting of the Board of Trustees on September 5, Clement Clarence Williams, since 1926 dean of the College of Engineering of the State University of Iowa, was elected president of Lehigh University. At the same meeting the title of president emeritus was conferred on Dr. Charles Russ Richards, whose resignation as president became effective on August 31. Dr. Richards became president of the university in 1921, succeeding Dr. Henry S. Drinker, who also retired with the title of president emeritus after serving as president for fifteen years.

IN connection with the sixth International Botanical Congress, which opened at Amsterdam on September 2, under the presidency of J. C. Schoute, professor of botany and director of the laboratory at Groningen, Dr. Elmer D. Merrill, retiring director of the New York Botanical Garden, now director of botanical work at Harvard University, and Dr. Donald Reddick, professor of plant pathology at the New York State College of Agriculture at Cornell University, were elected corresponding members of the Netherlands Botanical Association.

DR. CLARK L. HULL, professor of psychology at Yale University, was elected president of the American Psychological Association at the recent meeting at the University of Michigan. Dr. Harold Ellis Jones, director of research at the Institute of Child Welfare of the University of California, and Dr. Mark Arthur May, professor of educational psychology at Yale University, were elected members of the council of directors. Dr. G. W. Allport, of Harvard University, was chosen as representative of the association on the Social Science Research Council.

EDGAR S. NETHERCUT, secretary and director of the Western Society of Engineers for eighteen years, has been elected secretary emeritus. He is succeeded by Leigh S. Keith.

NAOHIKO MASAKI has been elected chairman of the board of directors of the Japanese Association of Museums. He succeeds the late Chiyomatsu Ishikawa.

PROFESSOR LUDOLPH BRAUER, for many years professor of internal medicine at the University of Hamburg and director of the Eppendorf Hospital, known especially for his work on tuberculosis, celebrated his seventieth birthday on July 1.

SIR FREDERICK GOWLAND HOPKINS, professor of biochemistry at the University of Cambridge and president of the Royal Society, and Dr. R. Inada, professor of medicine at the University of Tokyo, have been elected non-resident fellows in the division of biological, physical, chemical and natural sciences of the Paris Academy of Medicine.

PROFESSOR W. SCHULEMANN has been elected an honorary corresponding member of the Section of Tropical Diseases and Parasitology of the Royal Society of Medicine.

THE Johnson Memorial Prize of the University of Oxford for 1935 has been awarded to Dr. T. G. Cowling, Brasenose College, lecturer in mathematics at the University College of Swansea, for an essay entitled "Convection in Stars."

PROFESSOR C. W. CROCKETT, professor of mathematics and astronomy at the Rensselaer Polytechnic Institute, has been made professor emeritus.

PROFESSOR D. C. GILLESPIE has been appointed chairman of the department of mathematics of Cornell University. The retiring chairman is Professor W. B. Carver.

DR. L. N. DUNCAN, director of the agricultural extension service and a member of the administrative committee which has been in charge of the Alabama Polytechnic Institute, has been appointed president.

DR. WALTER MCK. MILLER, of Tufts College, has been appointed assistant professor of mathematics at the Massachusetts State College, Amherst.

M. H. FILSON, assistant professor of chemistry at Ohio Northern University, has been appointed head of the department of chemistry and physics at Mississippi Woman's College.

DR. HARVIER, until recently associate professor of therapeutics in the faculty of medicine of the University of Paris, has been named professor to succeed Professor Loeper, who has been elected to the chair of clinical medicine.

DR. K. MADDERS has been awarded a Halley Stewart fellowship in order to continue the research carried out under the direction of Dr. R. D. Lawrence in the Diabetic Clinic of King's College Hospital, London, and has also been granted clerical assistance for that purpose.

P. V. CARDON, since 1928 director of the Utah Experiment Station and since May, 1934, regional director of the land policy section of the AAA, in New Mexico, Arizona, Colorado, Utah, Nevada and California, has resigned to accept an appointment as principal agronomist in charge of the Division of Forage Crops and Diseases, of the Bureau of Plant Industry, Washington, D. C. He succeeds Dr. A. J. Pieters, who became head of the division in 1931.

DR. WILLIAM H. MARTIN was recently appointed director of research at the New Jersey Experiment Station. He will supervise research projects and budgets.

DR. HOWARD ROSS TOLLEY, professor of agricultural economics and director of the Giannini Foundation at the University of California, will return to the university after spending a year as head of the department of planning and research of the Agricultural Adjustment Administration.

DR. FRANCIS D. COMAN, of the School of Medicine of the Johns Hopkins University, who was medical director of the first Byrd Expedition to the Antarctic, has been given leave of absence and sailed on July 15 on an expedition to the South Seas. He will gather meteorological and other data for a projected commercial airline to the Antipodes.

DR. STANLEY P. REIMANN, director of the Lankenau Hospital Research Laboratory, Philadelphia, has gone to Germany to attend cancer conferences at the biologic laboratory at Oppau, near Heidelberg. He will also visit the Free Cancer Hospital in London.

JAMES M. WALTER, assistant pathologist of the Department of Agriculture, has left for England, where he will spend from two to five years studying the

Dutch elm disease. He will make his headquarters at Oxford.

MISS ANNEKE PANNEKOEK, paleontologist at the Geological Institute of the University of Amsterdam, lectured on August 13 before the Paleontological Research Institution at Ithaca, N. Y., on the geology of the East Indies, with special reference to the Miocene Molluska of Sumatra and Java.

DR. C. E. TURNER, of the Massachusetts Institute of Technology, has been appointed by the University of Calcutta to deliver a course of six lectures relating to the organization of health education.

THE fifth Victor Horsley Memorial lecture will be delivered by Sir Walter Langdon-Brown, Regius professor of physic at the University of Cambridge, on November 19 on "The Integration of the Endocrine System." The chair will be taken by Sir Frederick Gowland Hopkins, president of the Royal Society.

THE new Hayden Planetarium of the American Museum of Natural History, New York City, will be opened on October 3. Dr. Clyde Fisher, curator of astronomy, will be director of the planetarium and William H. Barton, Jr., will be assistant curator.

DR. FRED J. SEAVER, managing editor of *Mycologia*, announces the receipt of another gift of $1,000.00, which will be added to the *Mycologia* Endowment Fund held by the New York Botanical Garden. This is the second gift of one thousand dollars made by the same person, the name of the donor being withheld by request. It is hoped that when the interest on this endowment fund has reached sufficient proportions it may be used to defray the cost of special features of *Mycologia* which can not be met by the regular income.

The Experiment Station Record reports that following the granting to the governor of unusual authority in balancing the state budget, Michigan College has been granted for the ensuing biennium $1,284,653, as compared with $1,000,000 for the previous biennium. The agricultural extension funds were reduced from $178,609 to $166,250.

IT is reported in *The Museum News* that the Oregon state legislature has passed a bill designating the anthropological collection of the University of Oregon at Eugene as the Oregon State Museum of Anthropology. The bill provides that excavation and removal from state lands of archeological or anthropological material shall be under permit of the state land board and the president of the University of Oregon. Not less than half of all material so excavated is reserved for the state and is to be deposited in the State Museum at Eugene. The bill makes suitable provision for safeguarding the interests of museums, universities and other recognized institutions. L. S. Cressman has charge of the collections at Eugene.

A NEW agricultural building has been erected on the University of Hawaii campus. This two-story building, made of hollow concrete blocks, is 120 feet long by 60 feet wide and cost $68,000. It will house the offices of the Hawaii Experiment Station and offices and laboratories for agronomy, chemistry, plant pathology and soils in addition to those of the Agricultural Extension Division.

SATURDAY afternoon lectures at the New York Botanical Garden during September, October and November, beginning on September 7, are: September—"Wild Flowers of the Season," Dr. John Hendley Barnhart, bibliographer and administrative assistant; "Germination of Seeds," Dr. William Crocker, Boyce Thompson Institute for Plant Research; "Local Ferns," George T. Hastings, Theodore Roosevelt High School; "Dahlias," Dr. Marshall A. Howe, assistant director. October—"Fall Work in the Garden," T. H. Everett, horticulturist; "Autumn Coloration," Dr. A. B. Stout, director of the Laboratories; "Mushrooms, Edible, Poisonous, and Otherwise Interesting," Dr. Fred J. Seaver, curator; "Fungi and Our Food Supply," Dr. B. O. Dodge, plant pathologist. November—"Why Orchids Fascinate," Carl T. Ramsey; "Color in the Winter Garden," Henry Teuscher, dendrologist; "Interesting Plants of the South-east," E. J. Alexander, assistant curator and curator of the local herbarium; "Clematis for American Gardens," J. E. Spingarn; "Water Features in the Garden," A. C. Pfander, assistant superintendent. These lectures are delivered in the Lecture Hall of the Museum Building, illustrated by lantern slides and otherwise; free to the public. Doors closed at 3:30.

DISCUSSION

DEFINITIONS OF MATHEMATICAL TERMS IN GENERAL ENGLISH DICTIONARIES

A STRIKING instance of the shortcomings of certain definitions of mathematical terms in widely used English dictionaries is furnished by the fact that no satisfactory definition of the now widely used technical term group appears under the entry "group" in any one of the following four well-known works of

reference: Oxford English Dictionary (1888–1928), Century Dictionary (1911), New Standard Dictionary (1933) and Webster's New International Dictionary, second edition (1934). In none of the definitions in question is there any reference to the associate law which constitutes an essential element of every definition of an abstract group. This law is explicitly stated under the entry "group" in the preceding edition of the last of the four dictionaries just noted and its absence in the latest edition of this work marks a step backward which counteracts some of its forward steps.

Although the commutative law in the combination of elements was explicitly recognized much earlier than the associative law, the former being explicitly noted in Euclid's "Elements" (VII, 16) while the latter received little explicit attention before the middle of the nineteenth century, group theory has taught us that much more progress can be made by assuming the associative law without the commutative law than *vice versa*. The greater part of the developments in group theory result from the consideration of elements which are assumed to obey the associative law but not necessarily the commutative law when they are combined. The term associative law is due to the most noted Irish mathematician up to the present time, *viz.*, W. R. Hamilton (1805–1865), who was also the first to emphasize the importance of this law. A. Cayley (1821–1895) was the first to mention it in connection with a definition of an abstract group, but he was not steadfast in insisting on the fact that it is an essential element of the definition of an abstract group just as the last-named dictionary noted above displayed a lack of steadfastness in this respect.

A considerable number of new mathematical definitions appear in the second edition of Webster's New International Dictionary. The selection of the terms thus defined is not always wise. For instance, we find therein an incomplete definition of the somewhat special term "dicyclic group," but the much older and more widely used term "symmetric group" does not appear at the appropriate place. One also looks in vain in this dictionary for the entries "Fermat's theorem" and "primitive root." There appears, however, therein the entry "primitive group," but the definition which follows this entry is also incomplete, since in an imprimitive group all the substitutions which omit a given letter may also form a group involving all the other letters. Such incomplete definitions are very objectionable in a work which claims to be the "foundation book of education." Many readers will probably be more surprised to find therein also an incorrect definition of such a commonly used term as "spherical excess." Under this entry it is stated that its product by twice the square of the radius of the sphere on which the triangle is drawn is equal to the area of the triangle. The word "twice" should obviously not appear in this statement.

As this is the most recent extensive revision of any one of the large dictionaries noted above and hence will probably be used in our schools and elsewhere in its present form for a long time it may be desirable to direct attention here to a few additional points which seem to be in need of reconsideration. Probably most mathematicians would agree that the name of E. Galois (1811–1832) should appear in the "Biographical Dictionary" of this work in preference to many others which appear therein, such as that of W. Chauvenet (1820–1870), for instance. In fact, many would probably have preferred to see the name of E. H. Moore (1862–1932) retained instead of that of W. Chauvenet, in case one of them had to be omitted to make room for desirable additions. The name of S. Lie in this list should be followed by the dates (1842–1899) instead of by (1842–1879). The greater part of his publications appeared after 1879.

The preceding remarks do not imply that the existence of a few inaccuracies nullifies the value of such extensive works of reference as those noted above. On the contrary, they exhibit the great difficulties involved in the onward movement towards greater and greater accuracy. As regards many points it would evidently be impossible to secure now a unanimity of views, and the brevity of statements required in a work which deals with such a wide range of knowledge as a general dictionary naturally leads to some ambiguities. It is hoped that the points noted above are sufficiently outstanding to appeal favorably to the great majority of those who may consider them carefully and that this consideration will tend to a greater independence of thought while using such works of reference. Mathematics deals to an unusually large extent with finalities as regards accuracy, but it borders on fields in which such finalities are still impossible.

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THE EFFECTS OF ZINC SALTS ON THE OXIDATION PROCESS IN PLANT CELLS

SINCE 1870, when Raulin[1] demonstrated the remarkable accelerating effect of zinc salts upon the growth of fungi, the action of this and other "stimulating" or "accessory" elements has been the subject of many

[1] J. Raulin, "Etudes chimiques sur la vegetation." Paris, 1870.

investigations. Green plants almost invariably contain small amounts of zinc and it is probable that this element is essential for their best growth, although it is toxic, except at very low concentrations. The discovery in recent years of the extremely beneficial action of zinc salts on peach, orange, pecan and other trees affected with various types of little-leaf has opened new fields for study of this problem.

In presenting this note, we wish to call attention to several recent scattered publications on the specific action of zinc on cell metabolism.

It seems significant that several workers have arrived independently at the conclusion that zinc salts are intimately concerned with the processes of oxidation in the cell and with the transformation of carbohydrates in the plant.

Miss Colley[2] showed that zinc had more influence in promoting the growth of aerobic bacteria (*Pseudomonas tumefaciens*) in narrow test-tubes than in relatively shallow layers of liquid in Erlenmeyer flasks. Thunberg[3] published three admirable papers within the past year in which he showed that zinc salts produced a remarkable acceleration of oxidation processes when added to the extracts of certain seeds. Ganassini[4] believes that inorganic and organic basic zinc salts and zinc hydroxide may behave in alkaline medium as oxidative agents like the oxidases and peroxidases.

It is well known that the plant obtains a large part of the necessary energy from the combustion of sugars. Not all the energy obtained from this oxidation is used for growth. A portion, which we might term the "overhead," is used for the maintenance of the plant organization. The work of Javillier[5] indicated that zinc regulates the consumption of sugar in the cell. He thought that it diminished the amount of energy required for maintenance with a corresponding increase in the amount available for construction.

Confirmatory evidence on the relation of zinc to the consumption of sugars has been afforded by Haas.[6] He has shown: (1) that orange leaves affected with the disease known as mottle-leaf contained more reducing sugars than unaffected leaves; and (2) that the sugar content of mottle-leaf cuttings previously dipped in a zinc-lime-water mixture diminished more rapidly than that of mottle-leaf cuttings grown under similar conditions without any application of zinc.

The authors of this note have shown that the metabolism of cells of mottled orange leaves is profoundly altered. The pathological symptoms observed are related to an accumulation of suboxidized metabolic substances and a destruction of chlorophyll. The stromata of the plastids are often rich in fat globules and deficient in starch grains. The suboxidized metabolic substances were largely droplets of phytosterol. It is possible that sugars were converted into lipids instead of being oxidized to furnish energy to the cells. However, the normal course of metabolism in this type of leaf cell was resumed after the trees had been sprayed with a dilute solution of zinc sulfate. Chlorophyll was produced and starches and sugars utilized for the energy of the active leaf.

We have outlined recently the way in which zinc salts may promote oxidation through their action on sulfhydryl compounds.[7] This idea has been supported by Giroud and Bulliard's[8] demonstration of the specific action of zinc in stabilizing the nitroprusside color reaction of glutathione. We were able to show:

(1) An accumulation of zinc in the meristematic cells of buds and in the palisade cells of leaves.

(2) Renewed activity in the leaf cells as a result of the application of very dilute solutions of zinc sulfate as a spray.

(3) Accelerated growth of new shoots on trees subsequent to the application of zinc salts.

We can only hope that investigations in progress in various laboratories may afford information on the rôle of zinc which is greatly needed at the present time.

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PLIOCENE ANTELOPES OF THE PRONGHORN TYPE

THE phylogeny of the Antilocapridae and reported species giving rise to *Antilocapra americana* has been a subject of considerable interest to paleontologists. The work of W. D. Matthew on the *Merycodus* of Miocene and Pliocene age, and that of J. C. Merriam[1] on *Illingoceros* and *Sphenophalos* from the Pliocene, has increased considerably our knowledge of the probable line of descent of *Antilocapra americana*. The

[2] Mary W. Colley, *Amer. Jour. Bot.*, 18: 266–287, 1931.

[3] T. Thunberg, *Kungl. Fysiograf. Sällskapets I Lund Förhandlingar*, Bd. 4, Nr. 18, 1934; *Skandinavischen Archiv. F. Physiol.*, 69: 247–254, 1934; *Lunds Universitets Årsskrift, N. F.* Avd. 2. Bd. 30, Nr. 13, 1934.

[4] D. Ganassini, *Arch. Ist. Biochim. Ital.*, 3: 131–138, 1931.

[5] M. Javillier, *Ann. Inst. Pasteur*, 22: 720–727, 1908.

[6] A. R. C. Haas (paper in press).

[7] H. S. Reed and J. Dufrenoy, *Hilgardia*, 9: 113–141, 1935.

[8] A. Giroud and H. Bulliard, *Protoplasma*, 19: 381–384, 1933.

[1] J. C. Merriam, *Univ. Calif. Pub. Bull. Dept. Geol.*, 5: 22, 319–330, December, 1909; *Univ. Calif. Pub. Bull. Dept. Geol.*, 6 (Part 1): 2, 21–53, 1920, and 6 (Part 2): 11, 1911.

writer's attention was called by Merriam to a very interesting paper by E. H. Barbour and C. B. Schultz[2] in which they describe a new genus and species, *Proantilocapra platycornea,* Barbour and Schultz, from the Lower Pliocene in Cherry County, Nebraska. However, no comparison or reference to the closely related genus *Sphenophalos nevadanus* Merriam was made. *Proantilocapra,* as figured, shows morphologic characters close to those of *Sphenophalos.*

The horn-cores as to size, attitude in relation to orbit and frontal and in cross-section are much like those of *Sphenophalos.* The latter differ in being bifurcate, a character common to *Sphenophalos* from the Great Basin Province.

The occurrence of the new genus *Proantilocapra* in the Lower Pliocene of Nebraska adds important data in solving this problem.

In the original description and occurrence of *Sphenophalos,* Merriam discussed the systematic position of the genus and recognized, in the then available material, characters that indicate a close relationship to the pronghorn antelopes.

Specimens of the same species collected later in other Great Basin localities of middle and earlier Pliocene studied by Furlong[3] confirmed this view.

A more detailed report on the new genus *Proantilocapra* by Barbour and Schultz and other representatives of the same species will be welcome.

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CONCERNING REASONING

THE twenty-four canaries fly freely in the laboratory. One of them, Billie Burke, alit on the top of the window shade, slipped, slid down between the window and the shade, came out at the bottom, parachuted to the concrete floor. That accident the canaries have many times each day, but Billie was unlucky, caught her wing and broke it. There have been three injured wings in the seven years, but this wing healed badly and the bird will never fly.

While she was convalescing I put her water and food near her on the floor. The others have their food on the top of the zinc-top table. She disliked eating down there alone, and the first realization I had of the bad healing was seeing her pathetically inadequate efforts to spring to the top of the table. This spring was with her legs. She did not use her wings at all. The spring carried her in the beginning to a height of three or four inches, later to a height of seven or eight.

Presently it occurred to me to build her a spiral staircase of sticks around the one table leg, the sticks two inches apart. This I did at night. All the next day she paid no attention to the sticks. The other canaries paid no attention either, at least did not regard the staircase as a way to the top of the table, though they might perch on a stick or hop up two or three. So the second morning I decided to put out Billie's water but not her food. I watched all day for something to happen. Nothing did. Toward evening I tied leaves of lettuce to the ends of the sticks. Still nothing happened. The other canaries ate on the top of the table where they were used to eating, and Billie stayed on the floor and did not eat. Nevertheless, the third morning when I arrived at the laboratory she was on the top of the table, and from then on has lived most of her life on the top of the table.

But in a week or two I began to realize that even this was not satisfactory. Birds like to sleep high, and nights when they were all getting ready you could see Billie cock her head to watch the others. Therefore, what I did now was find a small tree, stripped it of all but a few of its upper branches, mounted the tree on the end of the table, tacked a staircase of sticks around the trunk, and in thirty seconds she was perched at the top! I had no experimental intention, of course. I was only wanting to get her up there, and there she was. It had taken her two days to see how to get to the top of the table, and thirty seconds to see how to get to the top of the tree. Yet the ladder round the tree in general appearance was certainly sufficiently different from the ladder round the leg of the table. How she originally got to the top of the table I do not know. It may have been by a succession of blunders, or it may have been blunder plus recognition, as is the more likely, considering that there were nine steps. But assuredly it would seem that she recognized the essential characteristic of her first experience, and used it instantly to a new end.

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SPECIAL ARTICLES

GRAVEL CUSPS ON THE CALIFORNIA COAST RELATED TO TIDES

WHILE living in Santa Monica during the winter of 1933–34, the writer became interested in the triangular masses of gravel and cobbles, called "cusps," which were found on the beach at the mouth of Santa Monica Canyon. It was observable that these cusps were subjected to great variation in size and number.

[2] E. H. Barbour and C. B. Schultz, *Amer. Mus. Nov.,* No. 734, pp. 1–4, August 3, 1934.

[3] E. L. Furlong, *Carnegie Inst. Wash. Publ.* 418, pp. 27–36, 1931.

They were not very conspicuous till after a large storm around New Year's Day, but from then on they began alternating between wide-spread development and almost complete extinction. Almost daily observations of their changes were obtained for several months in the later winter and spring.

Effect of Tide and Waves

Examination of the accumulated data showed a definite two-weeks cycle of cusp formation and destruction. Surprisingly enough, the occasional storms proved to have only a minor effect on the cusps and it appeared that the main factor in their change was the tides. During the neap tides, when the range between high and low water was least and when there was only one principal tide a day, the cusps became well developed both in size and in number. During the spring tides, when the range was large and there were two main tides a day, the cusps were either cut away or more commonly buried by sand. In general, the beach was eroded during the cusp-forming period and was built up with sand during the cuspless period. The same general relations were found at other beaches along the coast in the same vicinity.

In order to evaluate the effect of the tide and of the waves, a graph was prepared (Fig. 1), which shows

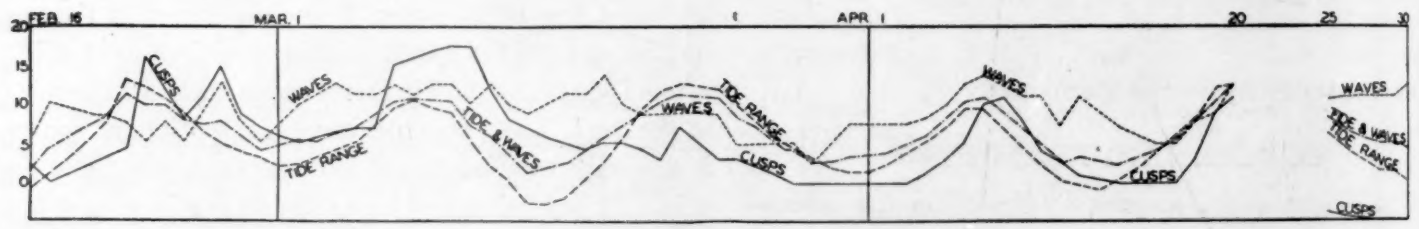

FIG. 1. Illustrating the relation between the area of cusps and the tidal range and wave size at the same times. The curves for wave size and tidal range are given in inverted form so as to correspond more with the cusps. The observations were made daily during most of this period. The curve given as "Tide and Waves" is a combination of these two factors weighting the tide range as twice that of the waves.

with arbitrary values: (1) the size of the waves (determined roughly each day); (2) the range of the tide during the preceding 24 hours and 52 minutes, and (3) the area covered by cusps. It appeared that large waves tended to eliminate the cusps in the same way that large tidal ranges produced this result. Accordingly, during some neap-tide periods the cusps did not develop to any great extent because of the off-setting effect of the waves. In order to show the relationship directly, the curve for the waves and the tide range is given in inverted form. Since the tides were thought to have approximately twice the influence of the waves, a curve was drawn combining the two influences in that proportion, and it will be noted that this curve follows quite closely to the curve of cusp area.

Causes of These Influences

The influence of the tide on the shores is presumably chiefly through the longshore currents which it produces. So far as could be told, these currents run from northwest to southeast (or west to east) during the flood tide and are not observable during the ebb tide. The winds cause currents in the same direction much of the time, so that the greatest currents are developed during spring tides when the west wind is particularly strong. Under these conditions the sand is driven along the coast and builds up the beaches covering up the gravel cusps. Evidently during periods of slack currents the waves come in directly onto the coast and cut away the sand, re-exposing the cusps. Johnson,[1] quoting Palmer, called attention to the fact that cusps are formed by waves "driven directly on the beach" and are destroyed by oblique waves. Somewhat the same principle is illustrated at Santa Monica.

Need of More Observations

It would be interesting to know whether the tidal cycles are important along other shores, either in connection with cusps or in the building up and cutting away of the beaches during neap and spring tides, respectively. So far as the writer knows, these cycles have never been reported in the literature. This may be because of failure to make enough observations or because conditions along the coast of Southern California are particularly favorable to the development of a tidal cycle, due to scarcity of storms and due to the diurnal tides which are found along the Pacific coasts, in contrast to the Atlantic, and make for greater differences between neap and spring tides when the diurnal tides correspond with the neap tides. Furthermore, it should be admitted that since the observations recorded here were not very protracted, it is conceivable that the tidal cycle which is suggested is purely coincidental. It is to be hoped that some scientists vacationing at the beach will have

[1] D. W. Johnson, "Shorelines and Shore Development," p. 457, New York, 1919.

the opportunity to gather more information on the subject.

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DEHYDRATION AND INFILTRATION

RECENTLY, in a series of papers too numerous to be cited here, there has been much discussion regarding the composition of fixing fluids designed to secure specific effects, mainly with plant tissues. The writer has done a great deal of similar experimentation, but the results have invariably been rather erratic and never wholly satisfactory. It was finally concluded that the trouble was caused by the fluids used following fixation and preceding infiltration with paraffin or balsam, and not to the fixing fluids themselves nor to the physiological condition of the tissues.

The tissues of all living organisms contain water and possess the capacity for absorbing water. The water contained in, say, vacuoles presumably does not react towards dehydrating fluids exactly as does the water in cell walls or in nuclei. The nature of the combination between water and the particular structure containing it varies according to the latter and may conceivably be altered during fixation.

Most technicians appear to take it for granted that the main if not the sole criterion of successful fixation is the apparent lack of changes during dehydration and infiltration. The writer, on the contrary, has become decidedly of the opinion that no blame can be placed upon the fixing fluid if disaster results during the post-fixation stages. In other words, "successful" dehydration and infiltration is not dependent upon "successful" fixation: the two processes are mutually exclusive.

The fluids commonly employed for dehydration are powerful desiccators. From the observation that the most satisfactory cases of infiltration resulted when only the free and not the combined water was removed from the tissues, it was apparent that it is possible to produce dehydration without causing desiccation. To state the case in another way: good infiltration occurs when the water is replaced, but the water-absorbing capacity of the tissue is not destroyed. The use of a fluid immiscible with water following, and in combination with, a dehydrating fluid which in itself produces desiccation only makes matters worse. Tissues become excessively hardened, plasmolysis occurs, and in the case of embedded material "cracking" results during sectioning on the microtome.

It seemed that what was required was a fluid miscible in all proportions with water, ethyl alcohol (in order to care for fixing fluids containing alcohol), paraffin and balsam and which would replace all free water yet cause no alteration in the water-absorbing capacity of the tissues. Practically all fluids in common use are automatically eliminated from consideration, as they fail to conform to one or more of these specifications. Normal butyl-alcohol comes nearest to conforming, but the anhydrous product is miscible with water to the extent of not more than 8 per cent. by volume; experience has already demonstrated that it was very unsatisfactory with many plant tissues, the reason being that desiccation had taken place. Dioxane, first suggested by Graupner and Weissberger[1] for use on animal tissues, and tertiary butyl alcohol[2] now appear to be the most promising reagents. Each is miscible in all proportions with water, ethyl alcohol, paraffin and balsam or xylol-balsam, as well as with most of the fluids in common laboratory use (except that dioxane will not mix with pure glycerin unless 10 per cent. water is added). An absolutely anhydrous product must, of course, be used to insure perfect dehydration.

Sufficient experience with both dioxane and tertiary butyl alcohol has already been obtained to indicate that, when properly used, these fluids: (1) Eliminate the use of ethyl alchohol and all fluids commonly employed to precede infiltration with paraffin or balsam. (2) Give perfect preservation of the fixation image. Plastids, mitochondria and similar cell constituents are preserved with remarkable fidelity, whereas such things are generally dissolved or otherwise rendered invisible by the usual combination of absolute alcohol and clearing fluid. No plasmolysis nor shrinkage results. (3) Remove all free water but leave the water-absorbing capacity quite unaltered, which makes it possible to soften hard woody tissues by simply exposing one cut end of the piece of embedded material to the action of water for a short time. (4) Produce no hardening whatever, thus permitting perfect microtoming. (5) The most delicate materials, such as *Volvox,* fern prothallia, moss protonema, filamentous freshwater and marine algae, and fungal mycelia, intended for whole mounts, can be transferred from water through either of the fluids directly into xylol-balsam diluted with that fluid and this evaporated down to mounting consistency with no shrinkage, collapse or hardening. Stains are perfectly preserved.

In the experimental work, a variety of fixing fluids

[1] *Zool. Anzeiger,* 96: 204–206, 1931. The writer, however, is indebted to Miss Enid A. Larsen, of the School of Biology, Stanford University, for information concerning, and the experimentation with, dioxane.

[2] The dioxane was obtained from the Eastman Kodak Company (#2144 1,4-dioxane), the tertiary butyl alcohol (a Shell Oil Company product) from the California Botanical Materials Company of Palo Alto, in whose laboratories it is being extensively used. The writer is not aware that tertiary butyl alcohol has previously been used for the purposes noted above.

have been used preceding the employment of either or both dioxane and tertiary butyl alcohol and many surprising results have been noted. Much of the obloquy heaped upon certain killing and fixing fluids appears to be wholly gratuitous and should be laid instead against absolute ethyl alcohol, xylol, chloroform, benzene and similar fluids employed for "clearing" purposes.

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IMMUNOLOGICAL REACTIONS AND VISCOSITY

IN 1923, while at the Rockefeller Institute in New York, we devised a microviscosimeter of high sensitivity[1] which enabled us to follow continuously the changes of viscosity occurring in a sample of solution, as a function of time, temperature or as a consequence of a reaction. It occurred to us that this instrument was well adapted to the study of immunological reactions—immune serum plus antigen—and that it might be interesting to find out whether flocculation and precipitation were not preceded or accompanied by some variations in the viscosity of the mixture. We found that such was the case, and that the addition of one drop of specific antigen to 1 cc of immune serum determined a considerable but *momentary* increase in the viscosity of the mixture. The amplitude of the phenomenon reached, in certain cases, 300 per cent. of the original viscosity. In a few minutes, the viscosity comes back almost to its former value. This reaction is strictly specific. Although we did not publish it at the time, we described the phenomenon at the Pasteur Exhibition in Strasbourg, 1923. In 1933 we reported it before the Congress of Immunology (Rome, Convegno Volta) and in 1934 in the Ergebnisse der Hygiene.[2]

Late in 1934, we took it up again with Miss V. Hamon and applied it to the study of the diphtheria toxin-antitoxin reaction which had been shown by Ramon[3] to yield quantitative results, making it possible to titrate the antitoxic activity of a serum *in vitro*. We found that, under the conditions specified by Ramon, who kindly supplied us with toxin and antitoxin, the same increase in viscosity could be observed, and that, for a given ratio of concentrations of the two substances, a quicker and more important phenomenon took place. It was thus possible to titrate the antitoxin content of a serum by a new and entirely different method, which may prove to be more accurate than the flocculation method.

The same method was then applied to precipitins by our associate, Dr. M. Coppo,[4] in our laboratory. It was found that the increase in viscosity in this case was proportional to the concentration in precipitins of the serum (rabbits injected with horse serum). The reaction is extremely sharp. A few interesting results were obtained: The higher the titer in antibodies of the serum, the smaller the quantity of antigen required to obtain the maximal viscosity. For instance, if 1.5 cc of immune serum precipitating at 1/1000 is mixed with antigen, the maximum of viscosity will be attained when .7 cc of antigen is added. If the serum precipitates at a dilution of 1/15,000 it will only be necessary to add .01 cc of antigen to 1.5 cc of serum in order to obtain the maximal viscosity. And in that case the absolute value of the maximum will be much higher than in the first case. In addition, it was observed that the rapidity at which the viscosity increases, then decreases, is a function of the concentration in antibody of the serum. It was also found that, for a certain and different ratio antigen antibody, corresponding to an excess of antigen, a minimum of viscosity occurred, the amplitude of which was much smaller than that of the maximum.

Similar maxima and minima, when the serum is mixed with tannin in definite proportions, were observed in our laboratory by another of our associates, Dr. F. Seelich. A detailed account of the experiments will appear shortly.

This new reaction seems, therefore, to raise questions of interest, not only from the practical, but also from the theoretical standpoint.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE USE OF THE PHOTOELECTRIC CELL IN PHYSIOLOGICAL EXPERIMENTS

THE photoelectric cell, as ordinarily used, measures the density of light to which its total active surface is exposed.

In searching for a method of conveniently obtaining a record of the motion of a beam of light, it occurred to us that the photoelectric cell could be made to record a continuous curve of the arc traversed by the beam, rather than simply the fact that it impinged or did not upon the surface of the cell. It was found that this could be done by interposing between the

[1] Lecomte du Noüy, *Jour. Gen. Physiol.*, 5: 329, 1923.
[2] Lecomte du Noüy, *Ergebn. Hygiene, Bakter., Immunit., u. Exp. Therap.*, 15: 304–334, 1934.
[3] G. Ramon, *C. R. Soc. Biol.*, 86: 661 and 711, 1922.
[4] M. Coppo, *C. R. Soc. Biol.*, 118: 1307, 1935; 119: 165, 1935.

beam of light and the cell a neutral wedge of glass, possessing a suitable, graded range of total light transmission from its thin to its thick end. Such a wedge was obtained through the cooperation of Dr. A. T. Williams, head of the Photometric Division of the Weston Electrical Instrument Corporation, who aided us in determining the proper values, and of Dr. H. P. Gage, chief of the Optical Division of the Corning Glass Works.

The light, sweeping across the surface of the wedge, reaches the cell with an intensity determined at each point by the graded thickness of the wedge. Thus, corresponding continuously with this modulated stimulus there is generated a progressively varying current, which can be led off to a reflecting galvanometer. A second beam of light, reflected from the galvanometer mirror to a camera, will trace the desired curve.

As the cell can be placed where it would ordinarily be impractical to place a camera, this constituted just the advantage which was desired in securing, simultaneously with the action potential and recorded on the same paper, a mechanical record of the contraction of a muscular organ situated in a somewhat inaccessible portion of the abdominal cavity.

Finding any modification of the usual lever or tambour recorder unsatisfactory, both because of the difficulty of attachment to the delicate, thin muscle without doing damage, and because of the lightness of the moving system demanded by the frailty of the muscle, optical registration was resorted to as meeting these requirements, and, moreover, possessing the necessary short period essential to an accurate tracing. Fig. 1 illustrates the set-up. The small supporting yoke, Y, made of heavy wire, has stretched between its extremities a phosphor-bronze or gold wire (such as used for galvanometer coil suspensions) to which is attached at the center, with rubber cement, a small fragment of a silvered cover glass. By means of the yoke this mirror is placed over the muscle and, after gentle torsion of the suspension wire, is allowed to rest almost flat upon the surface. A contraction wave, passing over the muscle, will rotate the mirror, causing a vertical excursion of the beam of light, MW. A system of mirrors necessary to bring the reflected beam from the preparation to the camera, and to convert the vertical motion into a horizontal one, adapted to the horizontal slit of the camera, K, proved to be tedious, necessitating readjustment with each experiment. It also resulted in a beam of light so long that the ensuing amplification made it difficult to confine the excursion to the camera slit, as well as requiring a very powerful source of illumination. The photoelectric cell as here described was, therefore, used. The cell, P, with the wedge, W, attached is placed about 6 inches from the mirror, M, which receives its light from a 50 candle power automobile bulb, L, through a condensing lens, C, also about 6 inches away. The galvanometer has its own source of light, L_2. The

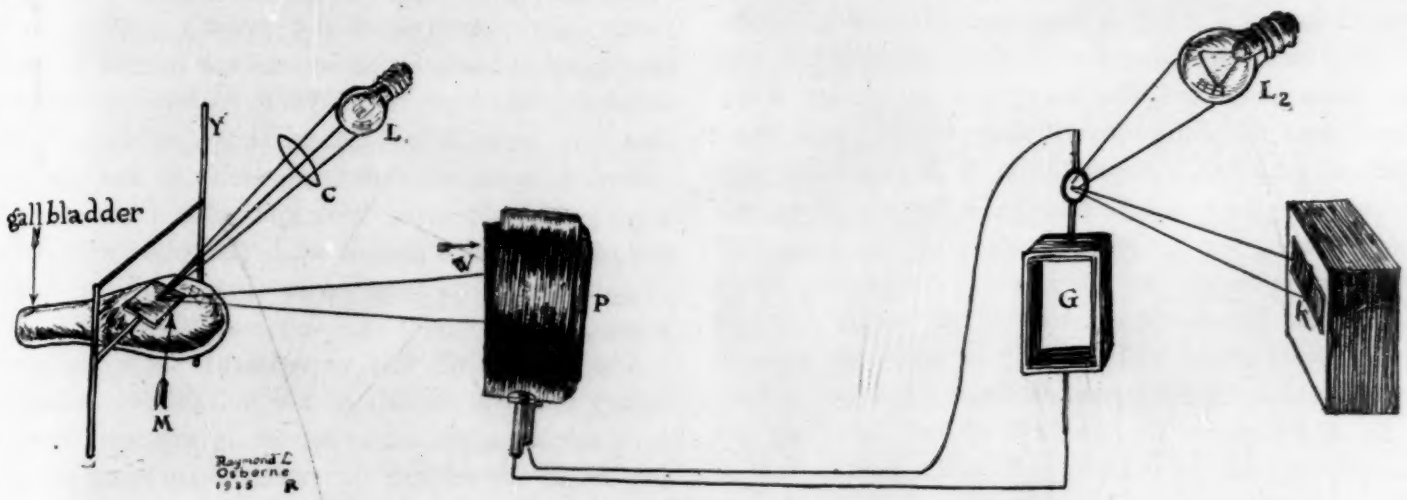

FIG. 1

cell used was the Weston Photronic. Fig. 2 shows a test curve made by manually rotating the mirror, M.

We feel that this arrangement renders optical registration for physiologic experiments a more flexible technique. The photoelectric cell, with interposed

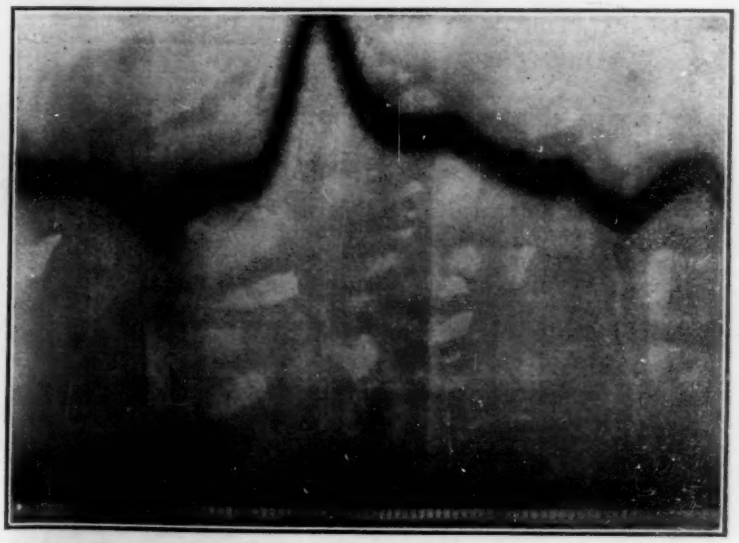

FIG. 2

wedge, can also be used to record the position or the motion of an opaque object in an illuminated field.

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SODIUM AMYTAL FOR ANESTHESIA IN STUDIES ON MITOCHONDRIA[1]

DURING the course of some studies on the mitochondria of the hepatic cell it became necessary to employ an anesthetic. As chloroform and ether are known to modify these cells[2] sodium amytal was tried. Eleven rabbits (New Zealand Whites), selected according to weight to five and three fourths to six pounds, received three grains of sodium amytal intramuscularly. Complete anesthesia usually occurs in fifteen minutes. After one half hour the abdominal cavity was opened and a portion of the liver excised and fixed. One other rabbit required six grains of amytal for anesthesia. The mitochondria of all twelve of the livers were compared to four controls killed at the same time and fourteen used in earlier work. There was no difference between control and anesthetized material. Accordingly, it is concluded that sodium amytal is a safe anesthetic for use in experimental work on the mitochondria of the liver, and when one considers the alteration of the osmotic qualities of the blood effected by ether it seems probable that amytal would be less likely to introduce error in investigations of mitochondria in any tissue.

In addition to the quality of not affecting the morphology of the mitochondria a single injection of amytal has the advantage of keeping the animal anesthetized for hours without harmful results, enabling the performance of prolonged surgical procedures without further dosage.

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A MODIFIED CULTURE JAR

AN experiment was conducted, in which 500 cc capacity inverted bell glasses were used as culture receptacles for soybean, Black Wilson variety, grown in Shive's[1] three salt R2S1 solution. Five hundred cc of fresh solution were supplied to each culture daily through a thistle tube.[2] Minute but equal quantities of iron[3,4] in the form of ferric citrate were added to the culture solutions of each series.

In series A, excess solution was siphoned from near the upper surface of the solution in the jar. In series B, excess solution was carried away from the bottom of the jar by means of a piece of bent glass tubing. Fig. 1 shows the arrangement of the jars in the two series.

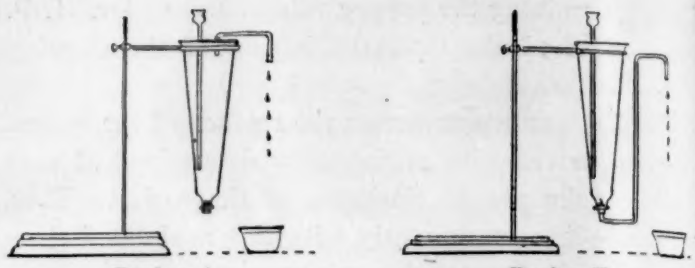

FIG. 1

The main difference in the technique employed in series A and series B was the method of draining away the excess solution. By draining it from the bottom of the jar, as in series B, the solution remained perfectly clear throughout the growth period of the plants, while the solution in series A became definitely clouded. The highest pH value recorded in the solution from series B was 5.4, while the highest pH value recorded in the solution from series A was 5.8. The total green weight of tops and roots from series A, per culture, was 4 grams, while the total green weight of tops and roots from series B, per culture, was 5.2 grams.

The results of this experiment show that more nearly uniform conditions are maintained in the culture solutions when the excess is drained from the bottom of the culture jar rather than being siphoned from near the top of the jar. Better plant growth also occurred when the solution was drained away from the bottom of the jar. This is due, probably, to the elimination of precipitate and plant wastes that could collect in type A jar, largely near the bottom, but were carried away from type B jar.

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